

Perspectives in high energy physics

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Abstract : A broad survey of High Energy Physics (HEP) both within as well as beyond the Standard Model is presented emphasizing the unsolved problems. In spite of the spectacular success of the Standard Model, there is a serious crisis facing the field. The importance of research on new methods of acceleration that can resolve this crisis by taking us to superhigh energies is stressed. We briefly review the status of HEP in India and offer suggestions for the future.

Keywords : Standard model, string theory, future of HEP

PACS Nos. : 11.15.-q, 12.60.Cn, 14.60.Pq

1. History

The major events which culminated in the construction of the Standard Model of High Energy Physics are presented in Table 1 in chronological order. Using nonabelian gauge theory with Higgs mechanism, the electroweak (EW) theory was already constructed in 1967, although it attracted the attention of most theorists only after another four years, when it was shown to be renormalizable. The discovery of asymptotic freedom of non abelian gauge theory and the birth of QCD in 1973 were the final inputs that led to the full standard model.

On the experimental side, the discovery of scaling in deep inelastic scattering (DIS) which led to the asymptotic free QCD and the discovery of the neutral current which helped to confirm the electroweak theory can be regarded as crucial experiments. To this list, one may add the polarized electron-deuteron experiment which showed that $SU(2) \times U(1)$ is the correct gauge group for electroweak theory, the discovery of gluonic jets in electron-positron annihilation confirming QCD and the discovery of W and Z in 1983 that established the electroweak theory. The experimental discoveries of charm, τ , beauty and top were fundamental for the concrete 3-generation standard model.

However, note the blank after 1973 on the theoretical side. Theoretical physicists have been working even after 1973 and experiments also are being done. But the tragic fact is that none of the bright ideas proposed by theorists in the past 25 years has received any experimental support. On the other side, none of the experiments done since 1975 has made an independent discovery. They have only been confirming the theoretical structure completed in 1973. It is clear that if such a situation persists for long, it may become difficult to continue to be optimistic about the future of high energy physics. We shall take up this point in Section 3.

Table 1. History of the standard model

Theory		Experiment	
1954	Nonabelian gauge fields		
1960			1960
1964	Higgs mechanism		
1967	EW Theory	1968	Scaling in DIS
1970			1970
1971	Renormalizability of EW Theory		
1973	Asymptotic freedom \rightarrow QCD	1973	Neutral current
		1974	Charm
		1975	τ -lepton
		1977	Beauty
		1978	$\bar{e}d$ expt
		1979	gluonic jets
1980			1980
		1983	W, Z
1990			1990
		1994	top

2. Perspectives and highlights of the symposium

The standard model based on the gauge group $SU(3) \times SU(2) \times U(1)$ describes *all* of presently known High Energy Physics. How well the standard model fits the data, was reviewed in the talks of Gautam Bhattacharyya, Somnath Ganguli and Atul Gurtu. This is the peak where we have reached. From here we can survey the view either below us (*i.e.* within the standard model) or above us (*i.e.* beyond the standard model). Possible topics in either view are the following :

Within the standard model :

QCD and hadronic physics

Higgs and symmetry breaking

Neutrinos

Generation problem

CP, axion *etc.*

Beyond the standard model :

Preons

Grand Unification

Supersymmetry and Supergravity

Higher Dimensional Unification

Superstrings

Let me first dispose of the view below the standard model.

QCD and hadronic physics :

Here the questions are the following :

- (i) Can we establish QCD to be the correct theory of strong interaction ?
- (ii) Can colour confinement be proved ?
- (iii) Can hadron spectrum be calculated ?
- (iv) Can hadron scattering be calculated ?
- (v) Do glue balls exist ?
- (vi) Does quark-gluon plasma exist ?

Ten years ago I talked on "Perspectives in HEP" (Ref. : Proceedings of VIII High Energy Physics Symposium, Calcutta, 1986, p. 399). The above list of topics and questions is in fact taken from that talk. Have the questions raised at that time, been answered ? In the following, I shall enclose the quotations from the 1986 talk as " "

"Unfortunately at the present moment, the answer to all these questions is negative. Answer to the first question will depend on the answers to the next three questions. Lattice-gauge-theorists are working hard on these problems. Here a word of caution may be appropriate, concerning the numerical calculation of hadronic properties such as their masses and couplings. It must be remembered that these properties of hadrons have been calculated earlier more than once in the history of high energy physics – first within the analytic *S* matrix and bootstrap approach and later in quark potential models. Each time success was claimed. The real test of any numerical calculation in hadronic physics must be the prediction of *a new number or a new phenomenon in the area of strong interaction*, which is then confronted with experiment. Until that is achieved, success cannot be claimed. After all, what is the sense of using expensive computer time to calculate the masses of the hadrons, when these can be obtained with much greater accuracy, by looking up the excellent Particle Data Tables ?" Although the main point of these critical statements still stands, one has to admit that important new developments have occurred. Asit De gave

a very lucid review of these and claimed that lattice QCD results are just starting to enter Particle Data Tables. This is good news !

"In the absence of a clean check of QCD in the realm of the dirty hadrons, the existence of glue balls or the transition of hadronic matter into quarkgluon plasma would be a direct and striking confirmation of QCD. But distinguishing glue balls from flavour-singlet quark balls has not proved a clean job. Let us hope that the imminent heavy-ion collisions will produce the eagerly awaited quark-gluon plasma and that the plasma will announce its arrival with a clean signal". Heavy-ion collisions have occurred, but people are still searching for clean signals of QGP ! C. P. Singh reviewed the current status of this field.

What about continuum QCD ? Light-front QCD appears to be a promising approach and progress in it was reported by *Harindranath*. A scholarly review on thermal field theory was given by *Samir Mallik*, who pointed out that the infra-red problem for finite temperature QED has been solved by *Indumathi*. The status of perturbative QCD and the structure function of the proton as revealed by HERA was reviewed by *Dilip Choudhury* and *Rahul Basu*.

Higgs and symmetry breaking :

"Is Higgs the correct mechanism of electroweak symmetry breaking ? There are claims from the axiomatic side that $\lambda\phi^4$ theory may be an inconsistent theory. Should Higgs mechanism be replaced by some other nonperturbative dynamical symmetry breaking ? In spite of much effort, we have not progressed much towards an understanding of dynamical symmetry breaking. Experiments being planned in the TeV region may reveal either the presence of Higgs bosons or a new type of strong interactions in the electroweak sector. In either case, we will have an exciting time". S. R. Choudhury showed how the triviality of $\lambda\phi^4$ theory combined with consistency can be used to yield bounds on Higgs mass and D. P. Roy described the ongoing searches for the Higgs boson.

Neutrinos, generations, CP, axion etc :

"Are the neutrinos massless ? If not, what are their masses and mixing angles ? The recent elegant explanation of the solar neutrino puzzle by *resonant* neutrino oscillations (the Mikheyev-Smirnov-Wolfenstein effect) must be noted. This explanation needs confirmation by independent experiments such as that proposed by Raghavan and Pakvasa (1987). Here one perhaps has a powerful tool for pinning down neutrino masses and mixing angles". The atmospheric neutrino puzzle has now joined the solar neutrino puzzle and both indicate neutrino oscillations. Neutrino physics has grown into an important field. Data from the new generation of neutrino detectors (Super-Kamioka, SNO and Borexino) are eagerly awaited. Also, long-base-line terrestrial neutrino experiments are being planned.

"How many generations of quarks and leptons exist and what fixes this number ?

Of the various options within the standard model for explaining CP violation, which is the correct one ?

Is Peccei-Quinn symmetry and axion the correct cure for the catastrophe of strong CP violation in QCD ? If so, where is the axion ?"

"On all these questions, enormous amount of theoretical work has been done, but no memorable results have come out. So most theorists have gone out of the standard model to make a living. This is not surprising, for this is what theorists have been always doing. We did not solve all the problems of atomic physics before moving on to nuclear physics, nor did we understand nuclear physics fully before inventing a new field called particle physics and moving into it. After reaching a peak we do not set up our permanent quarters there : we climb to the next peak. So, we move on to ... beyond the standard model." I then went on to describe Preons, SUSY and SUGRA, Higher Dimensions and finally Strings, which contained the following remark.

"Further, search for consistent theories of even more complicated objects than strings, for instance, membranes, lumps ... *etc* must continue. Any reported "No go" theorem in this context need not be regarded as a permanent barrier. Remember, without the invention of SUSY and acceptance of higher dimensions, even string theories would suffer a "No go" theorem. There will be discovered other things which will make the theories of membranes, lumps and even objects extending to higher dimensions consistent".

This is what has happened now. We are witnessing a Second Revolution in String Theory which has converted String Theory itself into a Theory of p-branes (objects extending to p dimensions).

Following are a few highlights of this symposium that dealt with "Beyond the Standard Model".

Supersymmetry :

Probir Roy, D. P. Roy and Ananthanarayanan presented comprehensive reviews of supersymmetric theories. We still await their experimental discovery.

String theory :

Sunil Mukhi gave a stimulating talk on the recent developments. Using the web of *duality* they are catching a rich harvest of interconnections between various string theories and they are already getting a glimpse of a so-called *M*-theory which may be the fundamental source of all string theories, membrane theories *etc*.

If string theory is the correct theory of Quantum Gravity it should help us to understand black holes better and the recent developments have achieved this. It is the understanding of the solitons and D-branes of string theory that has contributed to this development and *Dabholkar* dealt with this topic.

After listening to any talk on this Second Revolution in string theory, I feel so envious of my younger colleagues who are making such a fantastic progress in this difficult and highly competitive subject. (I wish I were 20 years younger !)

Two application of string theory :

(a) Proton stability :

The problem of catastrophically fast proton decay ($\tau_p \sim 10^{-5}$ sec) in supersymmetric theories, which is due to the existence of colour triplet scalars in these theories, is not yet solved. Conservation of R -parity is a possible solution and a few other solutions are technically possible, but not compelling. No deeper theoretical reason for proton stability has been found. *Jogesh Pati* argued that the real solution may require superstrings. Hopefully, this would provide the deeper reason.

(b) CP violation :

In an interesting talk, *David Bailin* sought CP violation in the orbifold compactification of 10-dimensional heterotic strings. It may be possible to incorporate CP as a geometrical transformation in a higher-dimensional theory and hence its violation may have a geometrical origin.

Dualized standard model :

In a beautiful work, *Tanmay Vachaspati* has shown how the standard model could be dualized. He starts with $SU(5)$ and breaks it down to a version of $SU(3) \times SU(2) \times U(1)$. The most remarkable aspect of his work is that no fermions are put by hand. The solitonic monopoles that arise in the theory have precisely the same magnetic charge as the electric charges on the quarks and leptons of the standard model. So, if we make the proper identification, the quarks and leptons can be generated as solitons ! This is certainly a *bolt from the blue* and deserves further study.

Topological quantum field theory :

Romesh Kaul described how the QFT framework (which we use to describe HEP) can be used to reveal the topological properties of 3 and 4 manifolds. Thus QFT has enough power to move the frontiers of Modern Mathematics too! In particular, duality in cohomological field theory leads to an almost trivial calculation of the famous Donaldson invariants in 4-D, which are in turn related to instantons. Since 4 is the number of physical dimensions of space-time in which we live and since Donaldson invariants are related to the infinite number of differential structures that have been proved to exist only in 4 dimensions, all this mathematics may have profound consequences for physics!

3. Does HEP have a future ?

We now return to the blanks in discovery mentioned in Sec. 1. The blanks have remained inspite of the tremendous activity in HEP in the past two decades. The biggest loophole in standard model is the omission of gravitation, the most important force of nature. Hence, it is now recognized that *Quantum Gravity (QG)* is the next frontier of HEP, and that *the true fundamental scale of physics is the Planck energy 10^{19} Gev, which is the scale of QG.*

We are now probing the TeV (10^3 GeV) region. One can see the vastness of the domain one has to cover before QG is incorporated into physics. In their attempts to probe this domain of $10^3 - 10^{19}$ GeV, theoretical physicists have invented many ideas such as supersymmetry, supergravity, hidden dimensions *etc* and based on these ideas, they have constructed many beautiful theories, the best among them being the superstring theory (or, *M*-theory, its recent incarnation), which may turn out to be the correct theory of QG.

But, Physics is not theory alone. Even beautiful theories have to be confronted with experiments and either confirmed or thrown out. Here we encounter a serious crisis facing HEP. In the next 10–15 years, new accelerator facilities with higher energies such as the Large Hadron Collider ($\sim 10^4$ GeV) or the Linear Electron Collider will be built and so the prospects for HEP in the immediate future appear to be bright. Beyond that period, the accelerator route seems to be closed because known acceleration methods cannot take us beyond about 10^5 GeV.

It is here that one turns to hints of new physics from Cosmology, Astrophysics & Nonaccelerator Experiments. Very important hints about neutrinos, dark matter *etc* have come from Astrophysics and Cosmology. Nonaccelerator experiments on proton decay, neutrino masses, double beta decay and 5-th force are important since they provide us with indirect windows on superhigh energy scales.

In spite of the importance of astroparticle physics and nonaccelerator experiments, these must be regarded as only our first and preliminary attack on the unknown frontier. *These are only hints !* Physicists cannot remain satisfied with hints and indirect attacks on the superhigh energy frontier. *So, what do we do ?*

As already mentioned, the outlook is bleak, because known acceleration methods cannot take us far.

To sum up the situation : There are many interesting fundamental theories taking us to the Planck scale and even beyond, but unless the experimental barrier is crossed, these will remain only as Metaphysical Theories.

It follows that either, *new ideas of acceleration have to be discovered or, there will be an end to HEP by about 2010 A.D.*

It is obvious what route physicists must follow. We have to discover new ideas on acceleration. By an optimistic extrapolation of the growth of accelerator technology in the past 60 years, one can show that even the Planckian energy of 10^{19} GeV can be reached in the year 2086 (see my Calcutta talk). But, this is possible only if newer methods and newer technologies are continuously invented.

Some of the ideas being pursued are laser beat-wave method, plasma wake field accelerator, laser-driven grating linac, inverse free electron laser, inverse Cerenkov acceleration *etc*. What we need are a hundred crazy ideas. May be, one of them will work. Lawrence's discovery of the cyclotron principle is not the end of the road.

4. Status of HEP in India and suggestions for the future

Theory :

There is extensive activity in HEP theory in the country, spread over TIFR, PRL, IMSc, SINP, IOP, MRI, IISc, Delhi University, Punjab University, BHU, NEHU, Guwahati University, Hyderabad University, Cochin University, Viswabharati, Calcutta University, Jadavpur University, Rajasthan University and a few other Centres. Research is done in almost all the areas in the field, as any survey will indicate.

Theoretical HEP continues to attract the best students and as a consequence its future in the country appears bright. However, it must be mentioned that this important national resource is being underutilized. Well-trained HEP theorists are ideally suited to teach any of the basic components of physics such as Quantum Mechanics, Relativity, Quantum Field Theory, Gravitation and Cosmology, Many Body Theory or Statistical Mechanics and of course Mathematics, since all these ingredients go to make up the present-day HEP Theory. Right now, most of these bright young theoretical physicists are seeking placement in the Research Institutions. Ways must be found so that a larger fraction of them can be absorbed in the Universities. Even if just one of them joins each of the 200 Universities in the country, there will be a qualitative improvement in physics teaching throughout the country. This will not happen unless the young theoreticians gain a broad perspective in the topics mentioned above and train themselves for teaching-cum-research careers. Simultaneously, the electronic communication facilities linking the Universities among themselves and with the Research Institutions must improve. This will solve the frustrating isolation problem which all the University Departments face.

Experiment :

Many Indian groups from National Laboratories as well as Universities (TIFR, VECC, IOP, Delhi, Punjab, Jammu and Rajasthan Universities) have been participating in 3 major international collaboration experiments :

- L3 experiment on $e^+ e^-$ collisions at LEP (CERN)
- $D\phi$ experiment on $\bar{p}p$ collisions at the Tevatron (Fermilab)
- WA93 & 98 experiments on heavy-ion collisions at CERN.

Highlights of the Indian contribution in these experiments were presented in this symposium.

As a result of the above experience, the Indian groups are well poised to take advantage of the next generation of colliders such as LEP2 and the LHC. Already the Indian groups have joined the international collaboration in charge of the CMS which will be one of the two detectors at LHC. It is also appropriate to mention here that Indian engineers and physicists will be contributing towards the construction of LHC itself.

Thus, the only experimental program that is pursued in the country is the participation of Indian groups in international accelerator based experiments. This is

inevitable at the present stage, because of the nature of present-day HEP experiments that involve accelerators, detectors, experimental groups and financial resources that are all gigantic in magnitude.

While our participation in international collaborations must continue with full vigour, at the same time, for a balanced growth of experimental HEP, we must have in-house activities also. Construction of an accelerator in India, in a suitable energy range which may be initially 10–20 GeV and its utilization for research as well as student-training will provide this missing link.

In view of the importance of underground laboratories in ν physics, monopole search p decay *etc.*, the closure of the deep mines at KGF is a serious loss. This must be at least partially made up by the identification of some suitable mine and we must develop it as an underground laboratory for nonaccelerator particle physics.

Finally, it is becoming increasingly clear that known methods of acceleration cannot take us beyond tens of TeV. Hence in order to ensure the continuing vigour of HEP in the 21st century, it is absolutely essential to discover new principles of acceleration. Here lies an opportunity that our country should not miss ! I have been repeatedly emphasizing for the past ten years that we must form a small group of young people whose mission shall be to discover new methods of acceleration.

To sum up, a 4-way program for the future of experimental HEP in this country is suggested :

1. A vigorous participation of Indian groups in international experiments, accelerator-based as well as non-accelerator-based.
2. Construction of an accelerator in this country.
3. Identification and development of a suitable underground laboratory for nonaccelerator particle physics.
4. A programme for the search of new methods of acceleration that can take HEP beyond the TeV energies.

Acknowledgment and Apology

I thank Dilip Choudhury for the invitation to give this talk and excellent hospitality at Guwahati. I apologize to those whose contributions could not be highlighted in my talk.